results by workers in both areas. The second is the related issue of the degree to which these results threaten the idea that generative grammars are neurologically instantiated.

For Müller, "autonomy" is strictly a claim about cognitive architecture. Drawing especially on the work of Fodor (1983), he characterizes it in terms of "informationally encapsulated and genetically prewired input systems with no feedback from higher cognitive systems and no interaction between input systems" (sect. 2). Let us call this autonomy₂. For linguists, in contrast, autonomy (autonomy₃) is a claim about how the grammars of human languages are organized. With respect to syntax, it is a claim that syntactic generalizations are optimally formulated in terms of a set of purely formal (i.e., nonsemantic and not discourse-derived) primitives whose principles of combination make no reference to syntax-external factors. With respect to grammar as a whole, autonomy₃ is a claim that language is a distinct system encompassing syntax, semantics, and pragmatics. The correctness of autonomy₃ stands or falls on linguistic evidence alone. Now, autonomy₃ has been advanced as a partial explanation for autonomy₃, and Müller presents arguments against the former. Unfortunately, since he does not distinguish between the two meanings of "autonomy," I fear that the conclusions of his paper might lead neurocognitive to dismiss generative grammar, interpreting the target article (false) to have refuted a central conception of this approach to language.

In fact, however, Müller may simply have provided an alternative explanation for autonomy₃. His notion of "regional functional specificity" realized by "distributive principles of representation" (sect. 6.2), when elaborated more concretely, might very well provide a neurological basis for the fact that grammars have the properties that they do. True, that would entail abandoning the strong Fodorian modularity claim, though I doubt that even many generative-oriented psycholinguists would go as far as Fodor does in "encapsulating" grammatical knowledge. In particular, Chomsky's conception of the autonomy of syntax and of language as a whole has always allowed for considerable interpenetration between the grammar and constructs external to it (see Chomsky, 1975).

It is worth pointing out that the broader view of autonomy₃ is shared by almost all linguists, including the three grammarians that Müller cites as congenial to his way of looking at things: Givón, Lakoff, and Langacker (see sect. 2). For example, Givón (1995; p. 176) writes: "The coding instrument called grammar has a well-documented, unimpeachable cognitive reality in discourse processing, a reality that can be manipulated and measured experimentally..." (it) likewise has an unimpeachable and well-documented neurological reality." The same can be said for the discrete linguistic objects that Lakoff and Langacker post ("idealized cognitive models" in the terminology of Lakoff, 1987).

Granting that I have no training in neuroscience, I must say that I was impressed by the evidence marshaled by Müller against a particularly strong view of the innate UC hypothesis. But it is a view that is shared by no linguist that I am aware of, not even Chomsky. For example, Müller supposes that an autonomous UG would have to be neurally contiguous (i.e., nondistributed) (see sect. 4.1 and note 15). But aside from some sketchy remarks on the role of the left hemisphere (Chomsky 1980, p. 240), Chomsky has never imputed to UG such strict physicalist or localist properties.

Some generativists, such as myself, have been more willing than Chomsky to seek an adaptationist account for UG (see Newmeyer 1991). Müller feels that my rooting UG evolutionarily in preexisting conceptual systems and preexisting perceptuomotor programs undercuts autonomy, which now "means little more than 'difference' and therefore simply does not match the concept as defined in generative linguistics" (sect. 3.1). I disagree. Difference, that is, governance by principles distinct from those of other domains, is all that has ever been meant by "autonomy" and, to the extent that the roots of autonomy lie in the human genome, suffices to characterize UG in the generativist sense of the term.

But are the roots of autonomy in the human genome? If we mean the broader autonomy of language, even Müller provides a qualified "yes": "Such gestural roots would be compatible with a gradual specialization of the neural basis of language during hominid evolution and human ontogeny" (sect. 3.1.1). But what about the autonomy of syntax? Müller goes 90% of the way to acknowledging a neural basis for it, while at the same time refuting a localist basis. That is, he agrees that "syntax is a nondiscrete cognitive specialization" (sect. 3.3.4) with "neuronal and regional specificity of function" (sect. 3.3.4) (in my view, evidence for a neural basis for autonomy), while noting that such specificity of function is compatible with "distributive principles of representation" (sect. 6.2). While such principles are, in theory, compatible with one interpretation of an innate UC, he rejects such an idea, writing: "Language areas develop epigenetically. They are end products of complex chains of interactions with internal and external environmental, which are] probabilistic events based on, but not rigidly determined by, the genome" (sect. 6.3).

Müller may be right about this, and he may be wrong. And, in principle, we know how to go about finding out. It is incumbent upon him and like-minded neuropsychologists to explain, for example, why certain highly abstract syntactic principles occur in every language and are evidenced by very young children. Or why English-acquiring children know that This is the paper that I filed before reading is a fine sentence, while I filed the paper before reading (where reading is transitive) is terrible, even though they have never been exposed to any sentences of that type. If Müller et al. can explain such phenomena by appeal to "chains of interactions," then well and good. If they cannot, then the idea of innate syntactic principles receives support.

But the basic point is that the linguists' search for generalizations goes on whatever the findings might turn out to be with respect to the neurological instantiation of the grammatical principles they posit. It could take a century or longer before we are able to provide an explicit linking between these principles and neural organization. Until then, indeed after then as well, the higher-level generalizations of linguistics will continue to provide input to the lower-level generalizations sought in neuroscience.

Neurobiology and linguistics are not yet unifiable

David Poeppel
Biomagnetic Imaging Laboratory, University of California San Francisco, San Francisco, CA 94143-0628. poeppe1@itsa.ucsf.edu; david.poeppel@rad-mac1.ucsf.edu

Abstract: Neurobiological models of language need a level of analysis that can account for the typical range of language phenomena. Because linguistically motivated models have been successful in explaining numerous language properties, it is premature to dismiss them as biologically irrelevant. Models attempting to unify neurobiology and linguistics need to be sensitive to both sources of evidence.

(1) Despite Müller's commitment to developing a neurobiologically motivated account of language, his perspective does not contribute substantively to a neurobiological understanding of language. His approach is perhaps more explicitly motivated by biological considerations than linguistically grounded models, but it is very underspecified. What is lost in the critical evaluation is any acknowledgment that something requires explanation, namely, the range of phenomena associated with language representation, acquisition, and use.

To be sure, it is well worth investigating foundational assumptions such as innateness or universality. But Müller's constructs should be connected to the phenomena of language, just as any model of language should be. What features of language will be captured by notions such as "cell assemblies" and "toposeman-
ticity?" Because his arguments are about the most general concepts - and not about how particular proposals do a better or worse job of explaining data on the representation, acquisition, and processing of language - it is difficult to evaluate what kind of phenomena Műller's model can account for.

It is disappointing that the categories generally used to describe language (e.g., concepts such as verb, morphology, inflection, and so on) are not addressed in a target article about language. Such concepts play an essential role in all successful attempts to explain properties of language. Something one might expect from a biological model is a way to talk about basic language phenomena. Trying to integrate the diverse biological information is important, but it is not sufficient; Műller owes the reader at least one example of how a particular aspect of language is best accounted for using the biological machine he has laid out.

This is not to say that the specific categories ("units") presently used in linguistic explanation are appropriate for biological explanations and will map onto biological categories in some straightforward way. It is possible that the categories proposed in current linguistics (phoneme, morpheme, phonological unit, affix, etc.) do not have obvious neurobiological correlates. Although it would be surprising if the brain represented language using categories markedly different from those that linguistics has used successfully to explain an enormous range of observations, it is naturally an open empirical issue what kind of linguistic categories are represented in the brain.

Analogously, neurobiologically motivated concepts such as cell assemblies or stimulus-induced "oscillations" are grounded in observations from neuroscience, and although they may do important work in terms of explaining certain biological observations, it is unclear whether these are the right biological categories to account for cognition.

(2) Műller presents the most uncharitable view of generative linguistics. If one is to believe his characterization of the field, the practitioners evidently suffer from a dramatic poverty of imagination, believing only in the most simplistic of biological models. To work in that framework of language research apparently commits one to beliefs such as single-gene-based genetic determinism, unitary localizationism, primitive one-to-one correspondences between brain areas and language modules, and a total lack of appreciation for plasticity and learning. This characterization is quite misleading. It is possible to be persuaded by innateness, autonomy, and universality without being naive about neuroscience. For example, the theoretical assumption that there are innately constrained parameters does not commit one to the belief that a single gene governs the construction of parameters in the biological system. The concepts of innateness, autonomy, and universality may have sometimes been used in a biologically unsophisticated fashion, but that does not imply that the concepts should be dismissed as biologically untenable. These concepts are not just a priori beliefs but are based on evidence. Innateness, autonomy, and so on are posited to account for data that would otherwise remain mysterious, not just to satisfy some theoretical predilections.

(3) Although the persuasiveness of foundational arguments underlies much of the initial appeal of generative linguistics, its continued appeal owes at least as much to the fact that linguistics provides detailed and explicit machinery that elegantly accounts for a wide range of language properties. As one example, one might consider some of the recent work on the representation and acquisition of verbal inflection (Phillips 1995). This research shows subtle similarities and differences among children learning various languages that could not be analyzed without the tools provided by linguistic theory. There does not yet exist a neurobiological vocabulary to discuss any type of category at the same level of detail, let alone subcategories relevant to explaining properties of verbal inflection.

(4) It is a fundamental goal of cognitive neuroscience to understand language in a way that is unified with our understanding of neurobiology (Chomsky 1993). But it is important to appreciate that neurobiology does not yet have the machinery to account for the range of typical linguistic phenomena. We may also have to seriously rethink the relevance and role of many neurobiological constructs in order to achieve unification. It hence seems premature to dismiss linguistic-theoretical concepts, even foundational concepts such as innateness and universality, as biologically irrelevant.

I share with Műller the desire to develop an understanding of language that is much more grounded in neurobiology. But as investigators of the biological foundations of language, we should acknowledge that at this point biology has far less to say about language than linguistics, warts and all.

Acknowledgments

Many thanks to John J. Kim and Sandeep Prasad for their helpful comments.

Biological language: Principle, predictions, and evidence

Friedemann Pulvermüller, Bettina Mohr, and Hubert Preissl
Institut für Medizinische Psychologie und Verhaltensneurobiologie, Universität Tübingen, 72074 Tübingen, Germany
pumeye@uni-tuebingen.de

Abstract: Műller's target article aims to summarize approaches to the question of how language elements (phonemes, morphemes, etc.) and rules are laid down in the brain. However, it suffers from being too vague about basic assumptions and empirical predictions of neurobiological models, and the empirical evidence available to test the models is not appropriately evaluated. (1) In a neuroscientific model of language, different cortical localizations of words can only be based on biological principles. These need to be made explicit. (2) Evidence for and against word class differences could be evaluated more rigorously. (3) All (and only) humans are able to learn languages with complex syntactic structures; it is, therefore, not appropriate to deny innateness and universality of syntactic principles. The real question appears to be the following: Which neurobiological principles are the linguistic principles based on?

Developing neurobiological models of language representation and processing is certainly an important endeavor. A summary of available approaches to the biology of language is, therefore, most welcome. A summary should, however, not gloss over the main points of the approaches summarized. In Műller's target article, it is sometimes unclear how assumptions relate to biological principles and how they could be evaluated empirically, while pertinent available data are frequently ignored. Finally, the article is unclear about why certain aspects of knowledge of language are only evident from human verbal behavior.

Neurobiological principles and word representations in the brain. "Content" or "open-class words" (that is, nouns, verbs, and adjectives) and "function" or "closed-class words" (including articles, pronouns, and conjunctions) are perhaps the two maximally different vocabulary types. It is hence intuitively plausible that these word types should have different neurobiological equivalents. According to Műller, "the acquisition of content word meanings is tied to environmental contexts," whereas "the context of acquisition for closed-class [function] words is much more reduced." The grammatical function of the latter is primarily inferred from a linguistic, sentential context. Analogous to this condition of acquisition, brain representation of closed-class [function] words is much less distributive than the representation of content words") (sect. 4.1). It is not clear how these statements relate to biological principles, hence they appear quite arbitrary.

A biological principle that may be relevant here is "Hebb's rule," according to which neurons that are frequently active together will strengthen their connections (Hebb 1949). In physiological experiments, synaptic strengthening after simultaneous activity of neu-